

ing us; and if we suppose our author to be sometimes insensibly influenced as to the weight of his authorities, by his preconceived opinions, the abundance of the details which he presents us, affords a ready corrective to the supposed partiality. Independently of the peculiar views which these volumes are designed to support, they have brought together a great mass of information of inestimable value, which it would be quite impossible to find elsewhere, except indeed by the same laborious process as that to which the author has himself resorted.

E. H.

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ART. XI. *Rapport et Discussions à l'Académie Royale de Médecine sur la Taille et la Lithotritie, suivis de Lettres sur le même sujet.* Par MM. DELMAS, SOUBERBIELLE, ROUCHOUX, CIVIALE, VELPEAU. 8mo. Paris, 1835. pp. 194.

IN the last number of this journal we offered some remarks on the work of M. BLANDIN, entitled *Parallele entre la Taille et la Lithotritie*, and we ventured to intimate, that with all its avowed candour, the author was evidently somewhat of a partisan. Nothing could tend to show the powerful, we might almost say violent party spirit at present prevailing in Paris, between the defenders of the two modes of operating to which we have alluded, than the debate in the academy, on the report of M. Velpeau.

Four long sessions were occupied in discussing, we know not precisely what—for the question at issue is nowhere defined with clearness. The debate winds up with “conclusions in which nothing is concluded”—for the report was finally adopted amid “incredible tumult,” and numerous protests against the manner of voting; M. LISFRANC demanding that the question should be put again at the succeeding session. There was much sharpness of retort and some little personality displayed in the debate, and among other after results, we may notice a warm discussion between MM. Velpeau and Civiale, of which we discover, as yet, but the beginning of the end. The damps of three thousand miles of ocean have a wonderful effect in cooling the warmth of an argument, and we shall endeavour to extract some few facts and conclusions from the several sources above enumerated, in order to illustrate the opinions at present entertained in France on the value of lithotripsy.

The memoir of M. LEROI contains the history of five cases in which the operation of lithotripsy was performed by him on children

at a very early age. The abstract as drawn from the report is as follows:—

1st case—a child of four years. *Stone* nearly an inch in diameter. It was very fragile, but required six operations for its removal. *Accidents.* Two fragments, at two different times, became engaged in the urethra, occasioning “a great deal of suffering.” *Result.* Cure. 2d case—age five years. *Stone* about the same size, destroyed by five operations. *Accidents.* None mentioned. *Result.* Cure. 3d case—age not noted. *Stone* about the size of a filbert, partially engaged in the urethra; seized and broken with forceps with three branches. *Accidents.* On the morning following the first operation, a fragment passed the prostate gland, became arrested in the urethra, and caused “violent pain.” Two days afterwards, another fragment was arrested in the same manner and produced the same consequences. It was repulsed with “infinite difficulty,” nor was it broken with less. *Result.* The child ceased to suffer, but as the patient was unmanageable, the cure could not be rendered certain by sounding. 4th case—age four years. *Stone* very small, and broken twice with great ease, the child being admirably manageable. Four operations are noticed in the report. *Accidents.* At the third operation, a portion of the forceps remained in the bladder, *a fact only known to the operator;* this was removed a few days afterwards by means of another forceps. A fragment of the stone was engaged at the same time in the urethra, and could not be pushed back immediately. *Result.* M. DUPUYTREN took charge of the patient, pushed-back the fragment, and performed the bi-lateral operation. The patient was thus cured. 5th case—age three years. *Stone* three or four lines in diameter. *Accidents.* None. *Result.* Cure after two operations.

M. Leroi, on this evidence, decides in favour of the possibility, but against the utility of lithotripsy in young children, except when assured that the stone is small. M. Velpeau coïncides with him fully in this opinion. We should go further, and object to the exception, believing that the attempt to assure ourselves of the size of calculi in the bladders of young children by means of any instruments known to us, would be little, if at all less dangerous than the operation of lithotomy, without proving always successful. *There are some questions in surgery which may be solved on plain principles of common sense, without the necessity of painful experiment.* But we anticipate.

M. Velpeau, not content with seconding the opinion of M. Leroi on this narrow question, has dared to open a wider field of discussion. He has attacked, in his report, the validity of the evidence

in favour of the general utility of lithotripsy, stating in good round terms, that the plan of breaking the stone deserves infinitely less eulogy than is generally bestowed upon it at present, and that in ten years from the present time, it will be practised much less extensively than at present.

"That which has given so much importance to lithotomy in the eyes of the world, is the fear of cutting instruments. It is the same fear that has made the fortune of caustics; of the *cura famis*; of compression in the treatment of cancers; of antiphlogistics, leeches, and divers highly praised local applications in lacrimal tumours," &c.

"Is it pain that they pretend to avoid by lithotomy? *The operation of lithotomy occasions infinitely less!* The same is true with regard to the duration of the operation, the danger of relapse, &c. If then lithotomy is a happy conquest of modern surgery, it must remain nevertheless, a method simply exceptional, when compared with lithotomy, after human reason shall be permitted to define its natural limits. Not only in children, but in adults also, it is attended by greater inconveniences than lithotomy whenever the stone is very hard or exceeds in size a walnut, (*gros noix,*) and when the patient has not too great repugnance to the latter operation."

After regretting the hallucination which he thinks prevents the lithotritists from perceiving the just weight of their own experience, M. Velpeau proposes two plans for estimating the relative value of the several methods. First, by comparing the mortality of calculus before and since the introduction of lithotripsy. "The work of M. Blandin, who alone has had the temerity to do so, proves already that in this point of view, experience pleads uncontestedly in favour of lithotomy." Secondly, by comparing the results of two equal groups of selected cases, in circumstances as nearly similar as possible, but treated on the two contending systems.

The report being presented, the academy commenced the discussion on the 5th of May. But one of the speakers, M. SEGALAS, attempted to defend the operation of lithotripsy in children; the main feature of the report passed therefore with very little opposition, and the debate took the wider range, which the bold, sweeping charge of M. Velpeau was calculated to provoke.

The principal speakers in opposition to the report were MM. AMUSSAT, SEGALAS and LISFRANC. Those who defended it with most warmth, were MM. VELPEAU, SANSON, SOUBERBIELLE, and PELLETIER du Mans. M. Roux held a station somewhat like that of moderator. We have availed ourselves in the following remarks not only of the publication which stands at the head of this article, but also the reports of the same debate in the *Gazette des Hôpitaux*, which contains a few remarks which have escaped attention in the

former. M. Amussat, who led the opposition, admitted that there was some ground for the charge of *exaggeration* made by M. Velpeau against the reporters of operations for lithotripsy, an operation which he did not advocate exclusively. But it is evident that he considered the exceptions rare, the operations improving, and that it promised to displace its antagonist in a vast majority of cases. He attacked also the statistics of M. Blandin, alluded to by the reporter, and advanced the opinion that it was impossible at present to obtain available details of this character because of the war existing between the partisans of the two methods. The other remarks of M. Amussat are rather ingenious than forcible—the mere opinions of BOYER and DUBOIS, however justly distinguished in their profession, are of less significance in a question of this character than the results of a few well observed cases, nor is it much more important that two eminent surgeons should have been preserved to society by lithotripsy while it continues debateable whether they would not have been saved with less risk by lithotomy.

M. Velpeau proceeded to state the accredited results, upon which were based his conclusion that the mortality from calculus in the hands of the friends of lithotripsy was greater than followed in the hands of the lithotomists. From this statement it appears that neither M. AMUSSAT, M. LEROI D'ETOILLES, NOR HUERTELOUP have ever published complete accounts of their results, and their success or failure in selected cases should weigh but little in a general question. There remain the summaries of MM. CIVIALE and BANCAL, together with certain private information communicated by M. Leroi, but which M. Velpeau did not feel warranted in stating, and some data of more ancient date. The evidence furnished by these is far from favourable in the opinion of M. Velpeau. He stated that of eighty-three cases *operated on* by the most able lithotritists, forty-two were cured, thirty-eight died, and of the forty-two cured, nineteen had serious accidents—a result which he acknowledged was “very different from that which appears on the face of the tables, (Raport Larrey.)” We shall speak of the causes of this discrepancy hereafter.

This terrible mortality, amounting to one death in a little more than  $\frac{2}{3}$  cases, will doubtless startle the reader! but the defenders of lithotripsy remark, that the operation has been vastly improved within a few years, and that it is unfair to include the earlier cases in a comparative estimate. M. Velpeau replies, that of twenty-four patients subjected to this operation at l'Hopital Necker, fourteen are cured, eleven are dead! Of forty-three (45?) operated on since

that period, fifteen are dead, thirty are cured; ten other cases retain their stone. Of thirty cases published by LEDAIN, in the *Gazette des Hôpitaux*, four retain their stone, eight are dead, and eighteen cured.

Rejecting the evidence of Bancal's report quoted by M. Velpeau, but evidently of no fair value in the premises, and the results drawn from M. Civiale, about which there is a warm dispute, which we must discuss in the sequel, we have still remaining sufficient data to show an apparent mortality of more than one in three!

A catalogue is then given of results in lithotomy, drawn from various authorities in Europe and America, which give results as follows. In England, the Continent and America, 5,873 cases of stone. Proportion of deaths one in a little more than eight.

A careless observer might well consider the question of the relative merits of the different methods decided, after such an expose as is given above; but let us take a single tabular result from the other party, and there are several such. M. Lisfranc, quoting the article of M. Begin in the *Dictionnaire de Médecine et de Chirurgie Pratiques*, states that M. Civiale lost but five cases out of two hundred and forty-four treated by lithotripsy! which after rejecting three cases in which the stone was not effectually removed, gives the proportion of 1 death in  $48\frac{1}{3}$ !

On the one hand M. Velpeau, drawing his information not from the tabular view of M. Civiale, but from his detailed cases and the report of M. Larrey, presents us with the following picture of the result just noticed. Total of cases two hundred and forty-four. Cured one hundred and thirty; dead or retaining the stone one hundred and fourteen! On the other, M. Civiale, in his letter, taxes M. Velpeau with misinterpretation, or neglect of the most important documents, furnished by him to the academy; while in the debate M. Amussat attacks the validity of the statistics brought forward in defence of lithotomy. How shall we explain this disagreement? The question must be met.

We are in the habit of placing the most implicit faith in the facts avowed by the learned and most industrious reporter, and we see no reason to question them in the present instance; but we are in the habit of weighing them strictly. More than once in the course of the debate, the imperfection and exaggeration of the reports of lithotomy are acknowledged by M. Velpeau, but he defends the propriety of meeting one exaggeration by another. He says, *and proves too*, what is so honestly acknowledged by M. Amussat, that the lithotriptists present their facts in false lights, and found upon them inaccur-

rate conclusions in favour of their peculiar method, (*Vide Bulletin in Gaz. des Hop.* May, 1835.) The same charge is obviously true with regard to the lithotomists, and M. Velpeau acknowledges its truth with regard to the data employed by him, finally relinquishing any proportion of deaths in lithotomy founded on his tables, and adopting one against which none of his antagonists can object; namely, one death in four cases. But in his statements of conclusions drawn from the documents of the lithotritists, his partisan leaning is still strongly obvious. He speaks as a debater, not as a philosopher. He states, with the evidence within his reach, that M. Civiale cured but one hundred and thirty cases after two hundred and forty-four operations, while that surgeon claims two hundred and thirty-six cures. How many of those one hundred and thirty cases died, according to M. Velpeau? We are not told. How many deaths resulted from causes not connected with the operation? We know not. How many failed from defect of instruments or imperfect methods of operation, from the disobedience of the patient, &c. &c.? All is dark! Of what value then is such evidence in settling the general question of the comparative merits of the contending methods? We answer unhesitatingly,—none whatever.

The contest between MM. Velpeau and Civiale growing out of this debate has just commenced. What light it may hereafter produce cannot be foreseen, but at present it does not tend effectively to illustrate the main question. The work of M. Blandin noticed in our last number, leaves it equally undecided, notwithstanding the assertion to the contrary contained in the report. The adoption of the latter by the academy after such a warm contest, shows only that in the opinion of a learned body in France, the advantages of lithotripsy have been somewhat over estimated by its partisans, without establishing its relative importance.

In closing our remarks upon the report of M. Velpeau, and the debate to which it gave origin, we will endeavour to enumerate the deductions which may be fairly drawn from them, and which are not devoid of importance.

1st. It is conceded on all hands that when the bladder and urinary passages are in a healthy condition; when there is no contra-indication from visceral or other disease; when there exists but one stone, and this is not very hard or rugose; when the calculus is not larger than a walnut, the patient being an adult, and when the operator has great mechanical skill combined with professional tact, and a thorough acquaintance with the operation, in preference to lithotomy, lithotripsy—*ought to be attempted.*

2d. If there be a plurality of calculi, within certain limits, the other circumstance remaining the same, lithotripsy is equally proper. This is a corollary of the last proposition.

3d. That in all cases, where the urinary organs are healthy, or nearly so, and when the simple methods of sounding employed before all operations in calculus fail in detecting an unusual hardness or size of the stone, it is warrantable to employ lithotritic instruments in the endeavour to ascertain its character.

4th. That examination by lithotritic instruments may render a patient insensible, (*Roux.*) or may involve his life, (*Velpeau.*) hence the comparison of equality made by M. Rouchoux, between this species of examination and simple catheterism, is unjust in the highest degree.

5th. That the slightest operations in surgery, such as staphyloraphy, or suture of the perineum after rupture in delivery, *may* produce fatal consequences; hence simple sounding is not *altogether* devoid of danger. (*Roux.*)

6th. That the operation of lithotripsy is sometimes attended with so little pain, as to be likened, by a patient who had been frequently subjected to it, to the extraction of a tooth. (*Amussat.*)

7th. That it may be regarded as questionable, whether the average amount of pain suffered in operations for lithotripsy, employed on the recent extensive scale, is in any degree less than that of lithotomy.

8th. That it is *at least* questionable, whether the serious accidents following lithotripsy are not more numerous than those following lithotomy.

9th. That it is not possible to determine with absolute certainty the completion of the cure in lithotripsy, but the proof of the removal of the last fragment may amount exceedingly nearly to certainty. (*Lisfranc.*)

10th. That the dangers of the operation may be much diminished by previous general treatment in some cases, and the pain may be sometimes lightened by previous catheterism, but this is not always the case, for the tenth session may be more painful than the first. (*ib.*)

12th. That it is impossible to determine by any statistical accounts now on record, the relative importance or success of the two methods, or to determine the same question by the first method proposed in the report.

13th. That the objection advanced by M. Amussat against the second method of ascertaining this point; namely, the cruelty and inhumanity of the test, is a *petitio principii*, and devoid of weight.

14th. That a comparison of success between an equal number of grave cases of calculus treated by the two methods, would be an injustice to the claims of lithotripsy; that a group of mild cases similarly employed would produce results unjust to lithotomy, and if a group of cases of a middle character were chosen, which would be accomplished with difficulty, the result would be an approximation to truth, dependent for its value upon the multitude and minuteness of observations *faithfully reported.*

These conclusions are all that we think can be fairly deduced from the academic debate, except one, which we shall notice hereafter; but if we may be permitted to include in the survey the correspondence mentioned at the head of this article, and the work of M. Blandin, already involved in the discussion, we should feel fully warranted in representing both the contending methods as still in their infancy. While the different lithotritists are contesting the merits of each other's instruments, (Velpeau,) and the various lithotomists, the claims of their several incisions, how can either method be regarded as even approaching perfection? We, therefore, think it quite possible, that an era may arrive, in which lithotripsy will enjoy a much higher rank among surgical operations than it can fairly claim at present. We may say the same for lithotomy. There is nothing in surgery incapable of improvement, and M. Velpeau's prediction, that lithotripsy will be held in much less estimation ten years hence, may yet fall to the ground.

Much was said, during the discussion, of the value of statistics in surgical questions of this character. They are, as M. Roux remarked, *of some value*, but the extent of that value depends upon the fullness of the details, and its depreciation, upon the misapplication of the evidence. If we dared to draw our conclusions of the relative merits of the two methods of treating calculus from the evidence within our personal knowledge, they would be considerably more favourable to lithotripsy than those of M. Velpeau. Of fourteen cases treated by lithotripsy by surgeons of our acquaintance, one died by causes totally foreign to the operation; one was subjected to an attempt which failed, owing to the excessive pain it occasioned, and the patient was afterwards cured by the lateral operation; and twelve were cured. The result of fifty-two cases of lithotomy in the Pennsylvania Hospital, as given in the table, (p. 97,) of this number, gives a proportion of 1 death in  $7\frac{2}{3}$ . It should also be remarked that the lithotritic instruments employed in the two first mentioned cases were of American manufacture, in some respects peculiar, and since judged imperfect. Eleven operations performed by Dr. RANDOLPH, of this city, have all

proved successful. In proof of the danger of the misapplication of statistical details, we will merely observe, that the table to which we have referred, establishes the fact that the mortality from lithotomy at the Pennsylvania Hospital, from 1756 to 1802, was 1 in 13 cases; from 1802 to 1822, 1 in  $7\frac{2}{3}$  cases; and from 1822 to 1832, 1 in 3. Yet those who know any thing of the institution would smile at the idea that these circumstances argued either a deterioration in the skill of the operators, or in the modes of operating.

A few words on the correspondence between the reporter and M. Civiale, and we shall close our remarks for the present.

It will be recollected that according to M. Velpeau, but one hundred and thirty cases were cured by the operation of lithotripsy, as performed by M. Civiale on two hundred and forty-four patients. The latter surgeon in his letter, which must be regarded as still higher authority than the report of M. Begin, already quoted, insists on the truth of the following statement. Number of cases operated on by lithotripsy since 1824, 244; cured, 236; deaths, 5; continue to suffer, though no longer labouring under stone, 3.

This monstrous discrepancy is greater than can be fairly explained by the acknowledged party leaning of the two observers. The charge of intentional misrepresentation is defeated at once by the high character of the opponents; and that of culpable ignorance of the documents, advanced by M. Civiale against M. Velpeau is utterly overthrown by the acknowledgment of the latter in his reply.

"At the academy, M. Civiale might have assured himself that if facts have been altered, it has not been by me; and that I am also acquainted with the memoir which he has inserted in the fasciculi of the academy, and it is precisely because he has taken the precaution to publish all his observations, that I have arrived at results in figures so different from his."

How then is the discrepancy explained? We have devoted no small care to the solution of this question, confining ourselves to the evidence before us. It appears pretty evident that M. Velpeau has founded his calculations upon the whole amount of evidence produced at various times before the academy, &c. including all the communications of M. Civiale, his publications, clinical records of hospitals, private letters, verbal information, personal experience, &c. &c. The public is furnished with his conclusions, but with only a part of his data.

As it regards the question at issue with M. Civiale, it seems that he has selected from the cases reported by the latter surgeon, two hundred and forty-four observations in which the operation of lithotripsy was, *in his opinion*, performed. The character of the commun-

tator is a sufficient guarantee for the fact, that of *these* two hundred and forty-four cases, but one hundred and thirty were *cured*. But there is an important breach in the chain of evidence. We are nowhere exactly informed what M. Civiale regards as a lithotritic operation, nor are we made certainly acquainted with the circumstances which he considers as a sufficient proof of cure. M. Civiale has defined what *he* considers an operation. "I have said, in the last fasciculus of the academy, where the exploration terminates, and where the operation begins."—*Letter to the Academy.*

The last fasciculus has not yet reached us, and we can only infer the opinion of M. Civiale, from the following passage in his letter, and from the remarks of M. Velpeau; but the inference is very plain.

"As to the pretension which has been advanced in some writings expressly designed to depreciate lithotripsy; to consider as real operations the preliminary examinations that are necessary to determine the condition of the patient, and to ascertain whether the operation can or cannot be performed; if any thing could surprise us, it is that this pretension should again make its appearance in the academy." *loc. cit.*

It is evident, from these passages, and the conflicting statements of the parties, that the two hundred and forty-four cases of operation, chosen by M. Velpeau, from the returns of M. Civiale, even if the acknowledgement of the former to that effect had been wanting, are not the same two hundred and forty-four cases acknowledged to have undergone the operation by the latter surgeon. We have then two series of observations re-entering and becoming confounded with each other at various points, and the conclusions drawn from one series can be of no value in estimating the accuracy of the other; the very attempt to use them for such a purpose is to a certain extent unfair.

The next question of any interest to the profession, is the value of the tabular result of each of these series considered separately, as elements of statistical calculations. That of M. Civiale's report, hangs on the validity of his definition of a lithotritic operation, and the nature of the manipulations exercised upon those patients whose cases fall without the definition. Let us hear M. Velpeau on this subject.

"As for myself, I have made use of the facts published by M. Civiale, thinking it impossible to draw them from a better source. Nevertheless, seeing that we build on the same foundations, how comes it that we differ so widely in our calculations? M. Civiale positively refuses to admit, that the examinations, the attempts that are made to recognise the existence of the stone, to seize it, or to break it with instruments, should be considered as operations. This is an

idea that we find in all his writings. Let us see, now, the nature of these preliminaries. The litholabe, the brise-pierre, or the purcussor, are introduced into the bladder, where one extremity is made to move about, in order to ascertain the existence and the situation of the stone. Then the instrument is opened; the branches are separated, to seize or embrace the foreign body, and to ascertain its dimensions and its form. Finally, an attempt is made to perforate, to crush, or to break in pieces the stone, by acting on the other extremity of the lithotritor, which is large and straight, in the urethra. It is repeated one, two, or three times, at intervals of a few days. It may now, perhaps, be demanded of me, in what respect the operation differs from these preliminaries. Truly, I know not! It has always appeared to me that, *once in the bladder*, the instruments expose the patient to as much danger when they manœuvre in the empty space, or fruitlessly, as when they act with real efficacy upon the calculus."—*Lettre de M. Velpeau.*

There is nothing in the evidence before us to contradict the truth of this picture—there is even something to strengthen it. However the advocates of a novelty may allow their vision to be obscured by their desires, the good sense of the profession will never permit it to sanction such definitions. It is in vain to pretend that there is any parallelism between simple sounding, as employed by lithotomists, and the incalculably more dangerous, though more accurate examinations of the lithotritists. Even catheterism by straight instruments, is not positively proved to be quite so *safe* as that performed by curved instruments, although we are quite prepared to defend its vast *superiority* under many circumstances. The urethra is not a straight canal—the force required to render it so, *may be* almost harmless, but this question is still *sub-judice*. Not so, straight catheterism, with instruments which distend the canal beyond its natural limits. The danger incurred in *these* is proved by many observations, and properly forms an element in all calculations of danger from operations requiring their use. We have authority to show that the simple dilatations preliminary to the operation of lithotomy, may be productive of fatal consequences. Such consequences occurred in the cases of MM. Michali and Chevals, (*Première Lettre M. de Souberbielle.*) How much more dangerous then, the complicated examinations in lithotripsy.

Death *may* follow simple sounding, as was very fairly argued by M. Lisfranc in the academic debate, but setting aside the extreme rarity of such a result, the fact may be neglected in calculating the relative dangers of the different methods of performing lithotomy, because this danger is incurred equally in them all; but when comparing the results of two methods having nothing in common, either

in the preliminaries, or in the after-steps, each must be charged with the ill consequences of every *necessary* manœuvre, whether diagnostic or therapeutical, or there can be no justice in the comparison.

From false views upon this subject, the tabular results of M. Civiale lose their value. With regard to those of M. Velpeau, they are sufficient to show a greater amount of failure in *attempts at lithotripsy* than we were prepared to expect—far—very far greater than we could have supposed probable from the known results of the few operations as yet performed on this side of the Atlantic! But as we know nothing of the mode in which the series of observations were selected from the mass furnished by M. Civiale, nor of the extent to which the latter may have extended his preliminary examinations beyond what we should consider their legitimate bounds; and as we are equally ignorant of the degree and nature of the failures, so called, in each individual case, we must confess that we cannot safely draw conclusions from the data.

The two letters of M. Souberbielle are replete with evidences of the misapplication and failure of lithotripsy, but the point most completely elucidated by them is, perhaps, the value of the assertion made by MM. Amussat and Lisfranc, that the general introduction of lithotripsy will lead to the earlier announcement of the existence of calculus on the part of patients, and hence, to the proportionate increase of the number who will apply for surgical aid before the stone or the urinary organs have fallen into such conditions as are calculated to render doubtful the propriety of the operation. The letters contain sufficient proof of the fact more than once alluded to by M. Velpeau, that stone may exist for many years, and may acquire great dimensions before its presence is even suspected by the patient.

The evidence of M. Souberbielle tends also to prove, that the number of cases which form acknowledged exceptions to the applicability of lithotripsy, bears a much larger proportion to the whole number of calculous affections than is supposed by MM. Amussat, Lisfranc, and other partisans of this operation. This, however, is a matter of minor importance. The only question really interesting to surgeons, is the degree of comparative danger attendant upon the operation in those cases in which its performance is considered warrantable and preferable to lithotomy. We have little sympathy with those who personify contending plans of treatment, and talk in glowing terms of “la gloire de Lithotritie.” There is a little too much of this disposition displayed in the academic debate. With us there is but one excellence in any plan of treatment—its utility. We rise fatigued and dissatisfied from the study of the debate and corres-

dence, with a strong hope that the labours of M. Double, in whose hands the details of M. Civiale's observations are placed, and the anxiously desired returns of MM. Anussat, Huerteloup, &c. &c. will soon place the profession in possession of more valuable data. At present, the cause of an operation, the extension of which we ardently desire and confidently expect, appears to be suffering almost as much from its friends as from its foes. We have been led thus prematurely into a long notice of this operation by its great importance in surgery, and the anxiety of the profession on the subject, and shall endeavour hereafter to keep pace with the current of events.

R. C.

ART. XII. *Illustrations of the Elementary Forms of Disease.* By ROBERT CARSWELL, M. D. Professor of Pathological Anatomy in the University of London. London, 1833-4. Parts 4, 5.

THE fourth and fifth fasciculi of Carswell's pathology sustain the impression expressed in a former number of this journal, (No. XXVIII. for August, 1834, p. 418,) that Dr. Carswell possessed the requisite ability and the peculiar cast of mind requisite for the arrangement of the morbid products under a systematic and natural classification. The fourth number embraces an account of those degenerations, which, from their peculiar colour, have been termed *melanic*. Melanoma is defined by our author as "a morbid product of a black colour, varying in its intensity, humid, opaque, and of the consistence and homogeneous aspect of the tissue of the bronchial glands of the adult."

As some difference of opinion exists among pathologists as to what productions should be classed under the head of melanosis, the work before us divides them into *true* and *spurious*; by this arrangement every morbid product resembling melanosis will be provided a location; while true melanotic degeneration will be more accurately and clearly defined.

True melanosis is a morbid product of secretion of a deep brown or black colour, presenting no evidence of organization; its form and consistence vary greatly, and are dependent upon the influence exerted by external agents. The most frequent location of this deposit is on the serous surfaces, and in the cellular tissue entering into the formation of organs; not unfrequently it will be found blended with the molecular structure of the organ, as by the process of nutrition; lastly, it sometimes exists in the blood, and particularly in that found